

passed through the greater number of changes appears to be the less altered. If this hypothesis is rejected, we must regard the schist either as a normal part of the Primrose Hill group, or a new formation. If the latter supposition be accepted, the number of Archæan formations in Shropshire is raised to three, viz.:—1. The Primrose Hill gneiss. 2. The Rushton schist. 3. The Wrekin volcanic group.

A geologist with a genius for speculation might conclude that we have in this small area the representatives of three of the great Archæan systems of North America—the Laurentian, the Montalban, and the Keweenaw. I would not contradict him, but I would respectfully hesitate.

#### VIII.—CRITICISMS ON RECENT PAPERS ABOUT FAULTS.

By Prof. J. F. BLAKE, M.A., F.G.S.

IN recent Numbers of the *GEOLOGICAL MAGAZINE*<sup>1</sup> has appeared a paper by the Rev. O. Fisher, "On Faults, Jointing, and Cleavage," which, it seems to me, should not be left to stand unchallenged. It is one of those in which mathematical symbols are made to do duty for arguments. Some idea is started, a few *W*'s and *P*'s are scattered about, an equation is written down, it leads to nothing, and then the conclusion is triumphantly reached. Sometimes, however, there is no conclusion at all; but statements come in incidentally which will hereafter be quoted with the introduction "I have shown." Surely there must be many geological birds too old to be caught by such chaff; but the "*MAGAZINE*" is also for the nestlings. Such papers, too, are otherwise harmful, for the wide-awake soon learn that credit may be gained by work unfinished, and speculations that are crude, and they are tempted so to seek it rather than by harder labour. So goes our science down and loses caste.

I trust, therefore, that the author will excuse me if I run full tilt at his production, and if he can return the stroke, and hold his ground, all the better for the spectators.

Part I. deals with "geometrical considerations." In the very first paragraph we are told to confound "vertical" with "perpendicular to the bedding," and are restricted to faults in strata with a uniform dip. This is very like the play of Hamlet without the Prince of Denmark. Then we are told that in direct faulting the beds on the whole are compressed vertically. This is only the case in the part where the fault is a common boundary, but "on the whole" every dislocation requires greater space in all directions—as may easily be seen by drawing a diagram of a dislocated brick.

There is no particular harm in § 1 and § 2, and the results are not again referred to. They are simply a few elementary exercises on the addition and subtraction of throws.

In Part II. we are supposed to have "The Mechanics of Faulting and Jointing." It starts off with the statement that "direct faulting is in many instances the consequence of settlement when the strata contract through solidification." I doubt if it is ever due to this

<sup>1</sup> *GEOL. MAG.* 1884, May No. pp. 204–213, and June No. pp. 266–276.

cause. The succeeding remarks, however, may be intended to prove that it is so, though it is very obvious they do not. In the first place, faults are essentially differential phenomena, and cannot be brought about by anything which affects the stratum as a whole. This may be seen in his Fig. 7; for if the pieces at the side were equally contracted, the gaps would be filled up and there would be no fault. Nothing is said about unequal contraction, so no reason is

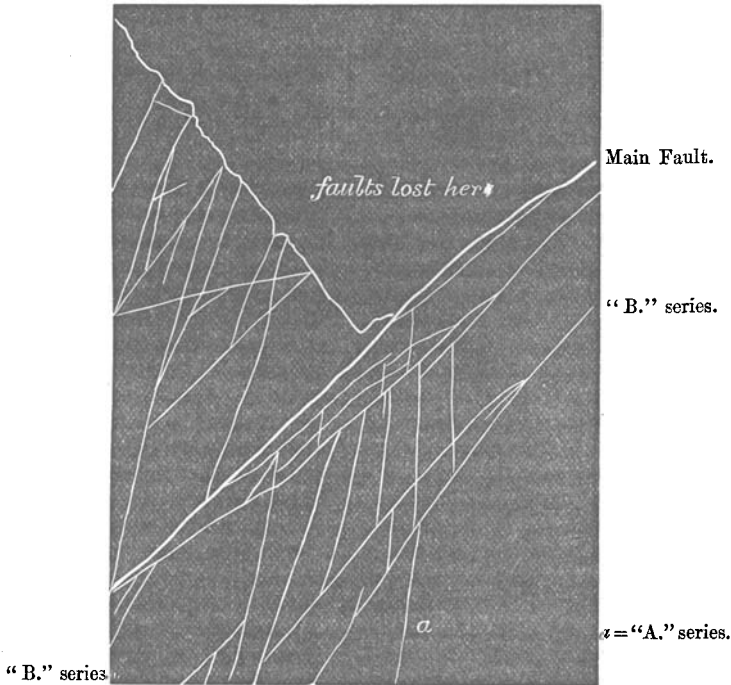


Diagram of Mr. Teall's Faulted Slate (see GEOL. MAG. Jan. No. Pl. I. p. 1, 1884.)

given why one part should contract more than another. This contraction theory is sometimes given for joints, of which it is a possible account in some instances; but the result of attempting to form faults this way may be seen by the remarkable Figures 8 and 9. In the first of these we have two faults crossing without dislocation of either! in spite of the correct relations having been given in Fig. 3. In the second we have a kind of mosaic of such errors, and finally faults dying out against an overlying stratum "which does not contract," and which was therefore supposed to be there when the faulting took place. Does any geologist know such faults? If not, it is of no use wasting time in trying to conceive how the bits in Fig. 9 were arranged before they all contracted. Certainly none such occur on Mr. Teall's slate. Here, however, we come to the end of this theory of faults. Is it proved? What has been done towards

that end? Nothing that I can see, except to confuse the reader to such an extent that he may find the acceptance of the theory whole, the best way to get out of it.

Next we start on quite a different tack to learn the "rationale of the mechanical action." Good bye, contraction! farewell, gaps! We now are to have a surface of shear and two forces at right angles—perhaps we shall get on better with these. Nothing like bringing in a  $\theta$ ! Meanwhile there is a dallying with plasticity. We are told that the assumption that the pressure varies as the area on which it acts introduces the idea of plasticity! Is a steam boiler then plastic? or a table on which a book of uniform thickness rests? However, it does not seem to matter; for we are immediately told what will happen "if it is rigid." Nevertheless, rocks cannot be "rigid" in a mathematical sense if they are to shear—for the definition of rigid is that they will *not* shear. Starting now with our  $WP\theta$  and  $\mu$ , we get an equation, and that is about all we do get. What light it throws upon the subject is not clear, but there is one peculiar feature about it. The  $\mu$  or resistance to shearing stress along a plane is made independent of the pressure perpendicular to that plane. Now is this so? Has it been proved? Friction, which comes into play when the rock splits, depends on the pressure; why not the resistance to shearing? If there are any experiments to prove this, of which I have never heard, it would be more instructive to quote them than Tresca's, which seem to have little to do with faults. But if this is not so, the whole of the mathematics fall to the ground. Without critically examined experiments to prove it, I should never believe that a normal pressure made no difference to shearing. On the next page there is an attempt to unite these supposed forces with the contraction spoken of before, but it leads to "joints" and not to faults, as might be expected, and  $\mu$  to be dropped and  $\kappa$  taken instead; as we know nothing about either, it does not much matter. We are led, however, to the remarkable conclusion that if a rock is cracked, the force which tends to crack it, is greater than that which tends to keep it from cracking! only it is put rather more scientifically (!) "The tension  $P$  . . . increases during contraction. Let  $\kappa$  be the cohesion per unit area of a vertical section. Then, on account of the great energy of molecular forces, we may expect that  $P$  is capable of increasing until it becomes equal to  $\kappa$ ." But it appears this tension  $P$  depends also upon the reaction of the fixed bottom. There is a sort of three-cornered duel, and as  $P$  beats both his adversaries, they must be equal between themselves! It would appear, however, from Fig. 7 that they are not equal, because at the bottom this contraction has been resisted; but at the top it has caused a separation. After the parenthesis, we get back to our equation, and it is made to show that faulting, if allowed, would always be ready to occur at  $45^\circ$ . There are, it appears, two conditions for faulting: one is, there must be room to move, and this is to be brought about by cracks. "Their formation" is "explained" by an equation! which we may suppose produces a suitable crack, though the only one mentioned is a vertical one; our fault, however, is one at  $45^\circ$ .

Does the fault stop at a joint? or what has this joint to do with it? Then we have horizontal joints! does any one know them in stratified rocks? What Prof. Tait would say to  $P$  being called a force and  $=\lambda x$  and then to the "force  $\lambda$ " being spoken of, I cannot think, —probably he would never read so far, nor indeed should I except for criticism, as I cannot see what is to be got out of it all.

Next there is a second condition for faulting, that is, "the hade of the fault must not be less than the angle of repose. (By hade here is meant the inclination to the horizon and not to the vertical, as is usual.) This marvellous proposition with respect to the case in which vertical pressure is alone supposed to act—the horizontal force having been spirited away—requires nine lines of mathematics! Who can doubt that if the upper mass is in "repose," it will not move? or, imagine that a force which is great enough to tear a rock will not move it when torn? How can tearing be shown but by motion? Then we have the following paragraph: [the remarks in brackets are Mr. Blake's.—EDIT.] "If the angle of repose is less than  $45^\circ$  [as it is for all known substances with approximately flat surfaces], the hade of the fault surface will be  $45^\circ$  [which is very rarely the case]; but, if the angle of repose is greater than  $45^\circ$  [which it never is, except the surfaces are hooked], the hade will be the angle of repose, provided it lie within  $lOm$  [hence vertical faults are impossible?]

Here we end the first half of the paper. Can we extract any ideas from it as to the *modus operandi* of faulting? No doubt the attempt will be unsuccessful, but this is what I gather. A mass of rock contracts: vertical contraction makes it sink, horizontal makes it crack; the total result may be an oblique fault, whose inclination to the horizon will be greater as the forces in operation are less. The position of the vertical cracks may be determined as follows. At the bottom, *i.e.* where the cracks end, the contracting force is resisted by the stress exerted by the bottom, which will be proportional to half the distance between the cracks; at the top it will be resisted by the cohesion of the rock; therefore the cohesion must equal the total bottom stress; or half the distance between the cracks equals the ratio between the cohesion per unit area and the coefficient of bottom-stress. The fault will first be started by the increase of the horizontal contracting force, and the easiest to make is one at  $45^\circ$ . There would be no room for the motion, however, and we must start again. The same force will pull across any crack, and will do this easiest when not resisted by the vertical force, hence it will make a vertical crack (!) When these cracks are made, the horizontal force will be exerted in making horizontal cracks [but this horizontal force is not the same as the other horizontal force, that one "might be a pressure or a tension," and "the tension arising from the contraction will amount to" it; but this one is a "contractile force" and "not a compressing force"!]. The only force then left to make faults is the vertical one, and this will make one at  $45^\circ$ , terminated by the vertical cracks, which somehow have turned into gaps.

The writer remarks that some former "suggestions" of his "are not generally satisfactory," and he probably says the same of the present by this time; indeed at the end of the paper he has found the beginning unsatisfactory.

We now start with the paper of June, and we read that "we have already seen that a direct fault must have a higher hade (to horizon) than  $45^\circ$ ." What is really stated is quoted above, viz. "the hade of the fault-surface will be  $45^\circ$ ," and faults with higher hade are only possible when the angle of repose is  $> 45^\circ$ . What the erroneous equations show is that it requires the stronger force to make the higher hade, which is manifestly contrary to experience. He now says "any fault with lower hade than  $45^\circ$  must be a reversed fault," which is also contrary to experience.

We next get to a new condition for faulting which, I suppose, should be equally true for direct faults, namely, that the shearing force must be greater than the friction, only in this case both horizontal and vertical components are used. As before, I should say, that this was self-evidently always the case, if by shearing force we mean a force sufficient to produce shearing, but this is not the case here. All that is done is to solve the following elementary problem. Given a crack and the coefficient of friction, what is the ratio between the forces for equilibrium? That this has nothing to do with the greater forces required for shearing if there is no crack, is seen from the results, namely, that less horizontal force will make a reversed fault (as distinguished from distortion) in clay than in solid rock, which is obviously false.

Fortunately the author at the close of this part sees that he is wrong; for he adds, "There can be no doubt that some of the most important faults are not produced by such a disposition of forces as we have contemplated."

Finally we have Part IV. on Cleavage. Here at least we have a definite idea expressed, and the mathematics, if not probative, are at least illustrative. The idea is that cleavage is brought about by the slipping of one slice over another with something like the motion of sand in an hour-glass. This is very like the old explanation of trough faults, only there are to be a great number of them. The author, however, says there can be no faulting, but he means *reversed* faulting, and seems to have forgotten his May paper. It is obvious that, if the middle of the anticlinal sinks fastest, the faulting will be direct, and hence, according to the author, will be more nearly vertical and perfectly possible. This is the only argument, and therefore the whole idea falls through. To start with two oblique lines, and to show that the intervening ones will gradually change over,—to imagine forces and write down the mathematical relations between them and get no further—these are not arguments, and cannot therefore be answered. Daubrée has indeed shown experimentally that cleavage may be produced by pressure which forces the mass *upwards*, unless the strain be relieved by faulting; if this is not strong enough to do it, *a fortiori* sinking down again would not be.

On the whole then, I am no clearer about Faulting, Jointing and Cleavage than I was before, but have had some difficulty in avoiding being puzzled on points which were perfectly clear before. Not so with a paper of M. Hebert, some time ago, which Mr. Teall refers to, but Mr. Fisher ignores. Though short, it had this point clearly brought out—that vertical pressure tends to produce direct, horizontal pressure reverse, faults; by this their relations to the districts in which they occur are clearly seen. I have been led to examine this paper of Mr. Fisher because it was apparently induced by the appearance of Mr. Teall's slate, on which I should like to say a word (see woodcut p. 367). It seems to me that instead of the several series of faults being formed, at different epochs, we have a clear illustration of the complex surroundings of one fault—the main one—when the compactness of the slate prevents its utter degradation into fault rock. I have copied the lines of faults as far as I can make them out, and it will be seen, that though the right-hand vertical fault (*a*) is a little shifted, yet others are not; the faults of the "*B*" set bifurcate on the right, and those of "*A*" on the left, and some appear to belong to neither set. In a word, they are all a series of minor faults, the fragments fitting as best they may, and the whole is very similar to those produced experimentally partly by direct pressure and partly by twisting, as figured in Daubrée's *Géologie Expérimentale*. It is to be noted that the bisection of the angles between the minor faults is pretty nearly perpendicular to the main one. Whatever the interpretation, the beautiful figure was a valuable new-year's gift to geologists, and is worthily placed as Plate I. of the new Decade.

## REVIEWS.

MEMOIRS OF THE GEOLOGICAL SURVEY OF INDIA, Vol. XX. Part 2; OR, GEOLOGICAL NOTES ON THE HILLS IN THE NEIGHBOURHOOD OF THE SIND AND PUNJAB FRONTIER BETWEEN QUETTA AND DERA GHÁZI KHAN. By W. T. BLANFORD, F.R.S., etc., Deputy Superintendent, Geological Survey of India. (Calcutta, 1883.)

**T**HIS Memoir has a more than usual interest as being the record of the last field-work undertaken by Mr. Blanford before his final retirement from the Geological Survey of India, after a service of more than twenty-seven years, during which he has not only enriched the publications of the Survey with a large series of valuable memoirs, but has also contributed most largely to our knowledge of the existing mammals, birds, reptiles, and land and freshwater molluscs of India and the adjacent countries.

The country of which the geology is described in this memoir is inhabited by turbulent frontier tribes, through whose territory it is necessary to advance with the protection of an escort, and in which there are some districts where it would be impossible to travel without a considerable military force. Under these circumstances, the movements of the geologist are considerably hampered; and as Mr.